iceberg is passed. In nearly every record we have there is a small rise of temperature above the surrounding sea temperature before the fall occurs, which seems characteristic of an iceberg effect.

In the light of the microthermograms we have obtained, the usual method of taking temperatures at sea seems decidedly inadequate. Thus, even if temperatures are taken over the side of a moving ship every fifteen minutes, readings are obtained at about two- to three-mile intervals, which obviously cannot be of much value in determining the temperature gradient characteristic of an iceberg. They might easily, as some of our charts show, indicate a rising rather than a falling temperature. The ordinary marine thermometer, with a degree one-eighth of an inch long, would miss entirely temperature effects made perfectly plain by the microthermometer. The persistence of a temperature gradient in the direction of a ship's course is one thing which I think can be relied on to give iceberg warnings, but when the whole temperature drop is fewer than two degrees in six miles it is evident that very sensitive thermometers must be used to detect it.

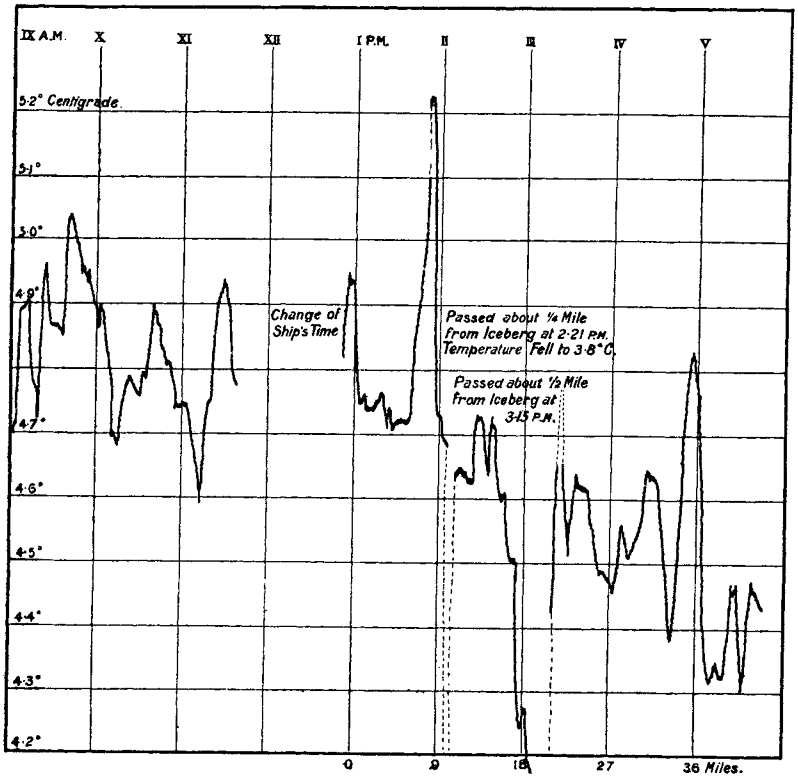

Fig. 2.—Microthermogram of the Temperature of the Sea.

Besides the disturbing influence of ice, the proximity of land within a few miles produces effects of great magnitude as compared with the remarkably small variations of temperature in the open sea. In our case this was, no doubt, caused by the cold under-currents being turned up by the shoals and shore line of the Labrador coast.

For hydrographic work, the determination of current boundaries could be made with great exactness from a comparison of the temperature traces and the determination of ship's position.

The small inequalities in the temperature of the sea stand out in strong contrast to the uniform temperature of the St. Lawrence River just after the ice has moved out in the spring. These inequalities of temperature suggest at once the possibility of a vertical circulation set up by convection currents, which must be an important factor in the retention of the solar energy absorbed by the sea. It explains why our records of air and sea temperatures show no diurnal variation, except in the former case when near land. It seems to be well known to biologists that small temperature variations in the sea may be set up by the existence of marine life, and it is difficult to think of any other cause for what we have observed. It is interesting as indicating how important a part marine life probably plays in the conservation of solar energy.

H. T. Barnes.

McGill University, October 27.

Dun Coat Colour in the Horse.

My attention has been directed to a letter in Nature of November 24 over the signature of Prof. J. Wilson. He disputes the accuracy of certain extractions from "The General Stud Book," which originally appeared in *The Veterinary Record*, in my paper on the inheritance of dun coat-colour. Prof. Wilson also states that in the Stud Book entries there is a considerable element of doubt. This would appear to be the usual attitude of his mind in relation to data which do not exactly fall in with his own theories.

Let me first take the case of the mare Silverlocks (foaled 1725). I most emphatically deny that "the Stud Book assumes" that this mare, which is described as a chestnut on p. 1, vol. i., is identical with a mythical chestnut mare which Prof. Wilson says was foaled exactly a hundred years later. The animal to which he probably alludes was foaled in 1824, and is entered in the third volume as a "*bay colt Silverlock*," by Blacklock out of Sheba's Queen. I would direct Prof. Wilson's attention to the fact that the chestnut mare Silverlocks (1725) is the only *mare* of that name in the first four volumes of the Stud Book, and that the Stud Book entry is perfectly authentic, since it was extracted from an early Racing Calendar which describes Silverlocks as a chestnut mare by The Bald Galloway, out of a sister to Chaunter. This mare was raced for some years, and there is no room for doubt that she was the dam of the dun colt Buffcoat, foaled 1742, and of his two dun sisters, foaled in 1738 and 1739 respectively, all three being by The Godolphin Arabian (brown or bay). Does Prof. Wilson mean to imply that Lord Godolphin was so dishonourable as to run Buffcoat under a false pedigree, for that in effect is what he would have us believe? I have already directed his attention in a private letter to the circumstance that I have a portrait of Silverlocks (1725) which shows her an unmistakable chestnut, and not a dun.

In regard to the dun filly Sarah Curran (1892), by Robert Emmett (bay or brown), out of Cellulites (black), Prof. Wilson is certainly misleading, for he fails to disclose the fact that Messrs. Weatherby distinctly state in vol. xviii., p. 727, that "this mare erroneously appeared in the last volume as dead." Now, whether Prof. Wilson likes it or not, the breeder of Sarah Curran, Mr. J. T. Hartigan, returned this mare as a dun. I judge he was in a better position to form an opinion concerning her pedigree and colour than my critic, who never saw her.

Prof. Wilson says that the filly (1886) by Lord Gough (bay) out of Danseuse (brown) is described as a bay. Here again he does not state the whole truth. As a matter of fact, this filly was returned as bay when a foal, but her breeder specially altered the colour to *light dun* in vol. xix. She had then reached maturity, and was a brood mare.

I do not wish to take up your space in quibbling as

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to whether *bay-dun* or *bay or dun* better describes the filly (1907) by Ash (chestnut) out of Unexpected (bay). Here, however, is another instance of reversion to dun which has come under my personal notice, viz. bay-dun hackney filly (foaled 1898), bred by the late Dr. W. Wingate Saul. She was by General Gordon, brown (gametic composition brown-chestnut); her dam, Fanny Gordon, a light yellow bay with black dorsal band. Fanny Gordon was by General Gordon out of Lancaster Fanny, dark liver-chestnut.

J. B. ROBERTSON.

Lancaster.

Lower Cretaceous Angiosperms.

IN the course of my work at the British Museum on Cretaceous plants, I have examined a number of more or less perfectly petrified "woods." Such specimens have generally been classed together as "Gymnosperms," so that they have received little attention from palæobotanists. As I am undertaking an exhaustive study of the Cretaceous plants, the keeper of geology has had sections made of all the likely specimens of woods.

Among those sectioned are Nos. V. 11517, V. 5654, and V. 5452. These are of exceptional interest, because they prove to be, not Gymnosperms, but Angiosperms. One of them is further notable in having its phlœm and cortex petrified, as well as the wood; the majority of silicified woods having lost these soft tissues. The specimens were collected at different times, which minimises the chances of error in referring them to the Lower Greensand; and from geological and petrological evidence there is no reason to doubt that they are, as labelled, of Lower Greensand age (*i.e.* the Aptian of the Continent).

I hope shortly to publish a complete and illustrated account of these specimens, but I make the discovery public now in the hope of obtaining further evidence. Hitherto the floras between the Wealden and Tertiary ages in Britain have not received much attention, owing to the very scanty and imperfect nature of the fossils representing them. Nevertheless, many collectors may have laid aside "wood" from the Greensands, Gault, or Chalk, and, if so, might be willing to lend them to me for examination.

So far as I am aware, the specimens, the nature of which I have recently determined, are not only the oldest Angiosperms from the north of Europe, but are the oldest from any locality with *petrified* structure. From the succeeding Albian, Fliche described an angiospermic wood —*Laurinoxylon albiense*—which he remarked was then (1905) the earliest known petrified Angiosperm. The numerous Angiosperms described from the United States and elsewhere from the Potomac and succeeding formations are leaf impressions only.

The existence of Angiosperms in northern Europe at so early a horizon as the Aptian is a fact which will necessitate revision in some current views as to the early distribution of the most important group of plants.

Manchester University. M. C. STOPES.

The Cocos-Keeling Atoll.

As a contribution from a frank supporter of Sir John Murray's theory of the formation of the lagoons of atolls by solution, I welcome the criticism of Miss Drummond (NATURE, November 24).

I must, however, point out that the discussion does not concern the question of the power of sea water to dissolve calcium carbonate, a fact which, so far as I know, is not doubted, but deals with the more special problem of whether this power for solution is the factor which has caused the development of atoll lagoons.

She has asked me a question, and I think that she has herself given the answer to it.

Taking the case which Miss Drummond presents, and accepting all her figures, we have the following facts. Normal sea water contains 0·12 gram of calcium carbonate per litre, and will form no precipitate on standing for any length of time. Sea water that contains more than this quantity (*i.e.* 0·649 gram per litre) will deposit calcium carbonate "in the crystalline form, and the deposition may go on until the solution contains less than is normally present in sea water."

In this last case, the saturation of the solution, in falling from 0·649 gram per litre to less than that of normal sea water, has passed through a stage at which it is represented by the normal 0·12. Therefore, in this case, sea water containing 0·12 gram of calcium carbonate per litre will precipitate calcium carbonate, and go on precipitating it until it contains "less than is normally present in sea water." This fact therefore negatives the first statement that sea water containing 0·12 gram per litre will not precipitate. What is the factor that determines the precipitation from normal (0·12) saturation in this case when, as Miss Drummond says, sea water when allowed to stand will not precipitate? I would suggest that it is the presence in the solution of the already formed crystals of calcium carbonate—a condition which is also present "in the interstices of the massive corals in the lagoons."

F. WOOD-JONES.

Conflicting Dates of International Congresses.

AT the request of the Swedish geologists, the International Geological Congress took place this year instead of in 1909. This year was also that in which the International Zoological Congress naturally fell to be held. Since, for the convenience of university workers, these congresses are usually held at the same time of year, and since they, with their excursions, now extend over a considerable period, especially in the case of the Geological Congress, it was almost inevitable that the times of the meetings should clash. This may not affect a large number of participants, but it is rather hard on palæontologists, whose interests lie in both camps, and who, even with the aid of the aëroplane, cannot be in two places at once.

I should not trouble you with a complaint about what appeared to be inevitable this year were there not signs of the same difficulty recurring in perpetuity unless protest is at once raised. As a matter of fact, the committee of "Palæontologia Universalis," when it met at Stockholm, forwarded to the council of the congress a request that this interference should be avoided in future. That protest seems to have been without result. If so, in 1913 the palæontologist will again find himself summoned either by duty or desire to opposite quarters of the globe.

F. A. BATHER.

The Megalospheric Form of *Ammodiscus incertus*.

THE interesting discovery of the megalospheric form of the above species in some abundance in the North Pacific Ocean, as described by Mr. J. A. Cushman in Bulletin No. 71, U.S. National Museum, 1910, pp. 73–5, and noticed in NATURE of September 1, brings to mind the remarkable occurrence of the megalospheric form only (*A. tenuis*, Brady) in some dredgings off Great Barrier Island, New Zealand, which I described in the Transactions of the New Zealand Institute in 1905 (1906). Curiously, the microspheric form was there entirely absent, although Dr. H. B. Brady had previously recorded it from a neighbouring *Challenger* station, No. 169. The latter author regarded *A. tenuis* as perhaps a local variety of the better known *A. incertus*. Rhumbler suggested that the form was possibly the megalospheric stage of the species, whilst the present writer, noting a large amount of variation in the initial chamber, suggested that a microsphere might be present in forms otherwise to be regarded as *A. tenuis*, giving the diameter of the initial chamber in the New Zealand specimens as 100 μ to 50 μ. Mr. Cushman's published figure shows an approximate internal diameter of the proloculum as 250 μ, which is nearer to Brady's published figures than to the examples from the Great Barrier Island. I am now convinced that the specimens from the latter locality had abnormally small megalospheres, giving the minima of measurements so far as known.

A question here arises how to account for the remarkable abundance of *Ammodiscus incertus*, clearly of microspheric relationship, in fossiliferous strata from the Upper Silurian to the late Tertiary. With that problem as a suggestion for observant rhizopodists I conclude this note.

F. CHAPMAN.

National Museum, Melbourne, October 20.

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